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SOME METALLURGY RESEARCH IN NORTHERN ITALY

By HARRY A. LIPSITT

29 March 1968

UNITED STATES OF AMERICA

17

TABLE OF CONTENTS

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SOME METALLURGY RESEARCH IN NORTHERN ITALY

INTRODUCTION

Science and science based industries are mushrooming in Italy. This is especially true in the heavily industralized North. This report covers a visit to Milan and environs. It is the first report on metallurgy in Italy since that of Walter Beck in 1964 (ONRL-57-64, 25 November 1964). On this trip I was able to visit three institutions:

CISE: Centro Informazioni Studi Esperienze, Segrate, Milano;

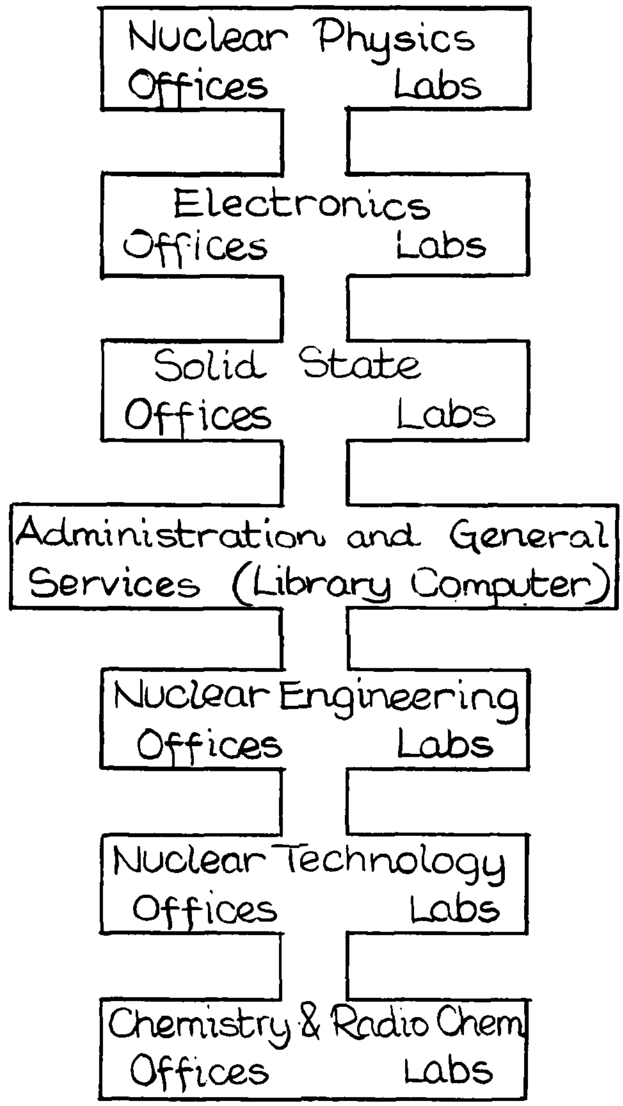
Laboratori di Elettrochimica del Politechnico di Milano;

Euratom: Centro Comune di Ricerche, Ispra, Varese, Italy.

CISE: Centro Informazioni Studi Esperienze

Acronymania (with apologies to New Scientist, 29 February 1968) has reached Italy. One must only remember that Italian is completely phonetic; CISE is pronounced cheesa. A rough translation of the name of this institution gets you exactly nowhere, which is precisely what the founders (some power companies of Italy) had in mind. CISE was founded in 1946 as a private non-profit laboratory for pure and applied energy research. They were really interested in studying the peaceful uses of atomic energy (nuclear energy for the production of electricity or energy), but in 1946 the Italian government was not allowed to have anything to do with nuclear energy and so the nuclear energy part of the research had to be carried on under a barrel. And so this is why the title CISE means nothing to us.

In 1947 CISE had a staff of seven, in 1954 they had grown to 70, in 1961 when they moved into their present quarters they were 170 and now they are about 350. The organization originally consisted of six divisions, three basic and three applied. The nuclear physics, electronics and solid state physics were the three basic ones: Nuclear engineering, nuclear technology and chemistry and radio chemistry were the three applied ones. The entire organization was housed in a unique, fully integrated structure, illustrated on the next page:



In 1963 the government of Italy nationalized the electrical industries. CISE was given the choice of becoming wholly government supported and therefore government run, or becoming wholly private. They chose to be a private research organization -- a contract research organization. As a result of this, there has been reorganization and there are now simply three divisions.

One is the nuclear plants division which incorporates all three of the previously applied divisions. The new electronics division incorporates most of what was electronics and all of what was solid state. The new physics division has the old nuclear physics, atomic physics and that part of the electronics section that did apply to nuclear and atomic physics.

My main contact was Professor A. Ascoli. Ascoli's service section is mainly responsible for specialty materials for electronics, but in addition he has wide freedom to do basic research providing that basic research is paid for under contract. He has seven people on a permanent basis in his group and one guest whose salary is paid for by the National Research Council of Italy (CNR). The research of that guest is one of the things I will be writing about later on.

I was curious to know why Ascoli was teaching (this is a fairly common thing for industrial people in Italy to do). He gave me four reasons: first of all, he liked it; second, teaching provides a non-negligible part of his income; third, he retains valuable connections with the university; and fourth, he is able to retain his professor's title, his libre docent, which is a professor's degree (roughly equivalent to our PhD) and allows him to use this title.

His section is developing new materials in support of the research of the electronics section. They are learning to grow crystals of II-VI compounds and to study their properties. They are trying to grow ZnTe and mixed Zn (Se, Te) crystals. In addition, they are growing crystals of ammonium diphosphate and potassium diphosphate. (These are insulators and optical crystals that are used for laser work.) They are also beginning now to look at several niobates.

Ascoli has been using precision resistivity techniques to study the formation energy of lattice vacancies and the energy of vacancy-impurity atom interactions. This research has been supported by the US Army Research and Development Group through their office in Frankfurt, Germany. The early results have appeared in print (Acta Metallurgica, 14, p. 1002-1005, 1966). That paper gave the energy of formation of a vacancy in Ag as $E_f = (1.06 \pm 0.05)$ eV. The change in resistivity at any temperature may be used to calculate the vacancy concentration at that temperature (if the resistivity change due to 1 atomic percent of vacancies is known). This calculation gives an equilibrium vacancy concentration at the melting point some 4-9X as large as that derived from quenching experiments. This is perhaps because the quenching studies assume that the change in resistivity due to vacancies as measured at 20°C is equal to the resistivity change caused by those same vacancies at equilibrium at a temperature near the melting point.

That resistivity is not linear with temperature to the melting point is shown by their data which possess extraordinary precision at elevated temperature.

A paper giving these data for Au and Cu is now being written. The energies they have determined for vacancy formation in these two metals are: E_f (Au) = 1.02 ± 0.07 eV, and E_f (Cu) = 0.92 ± 0.11 eV. They have nearly completed a similar study of Au doped with very small amounts of Cu. They have used three alloys: Au - 0.05 a/o Cu, Au - 0.5 a/o Cu and Au - 1.0 a/o Cu. The apparent energies of vacancy formation in these alloys are 1.00 ± 0.01, 0.91 ± 0.02 and 0.92 ± 0.02, respectively. They have derived an expression to handle the case where there are both free and bound vacancies. This expression shows that some function of the change in resistivity due to one concentration of impurities divided by the same function for another impurity concentration is proportional to an exponential in the binding energy. This expression is valid for use only within about two hundred degrees of the melting point of the material and also when there are only a few impurity atoms present, usually less than 1 a/o. Using this expression, they have determined the binding energy between a Cu atom and a vacancy in Cu doped Au as E_b = 0.25 ± 0.02 eV. They have similar values in Ag doped Au (E_b = 0.30 ± 0.01 eV) and Pd doped Au (E_b = 0.40 ± 0.18 eV). They have obtained a preliminary value of E_b for Pd doped Ag, but it is very small and must be checked further.

The bulk of their present work involves the use of a microcalorimeter to check the values for E_f already determined using the resistivity technique. The calorimeter has a sensitivity of 10^{-4} cal/hour and is useful to 1000°C. The technique used is to determine the heat capacity of a specimen as a function of temperature. If the material were stable with time the change in temperature versus time should be a simple exponential. If, however, vacancies are annealing out during the cooling of the specimen, the energy associated with the vacancies also appears as a deviation from simple exponential behavior. The magnitude of the deviation may be used to compute E_f. This has been done by Ascoli only for Na thus far; the value obtained was E_f = 0.48 ± 0.04 eV which is in good agreement with existing data.

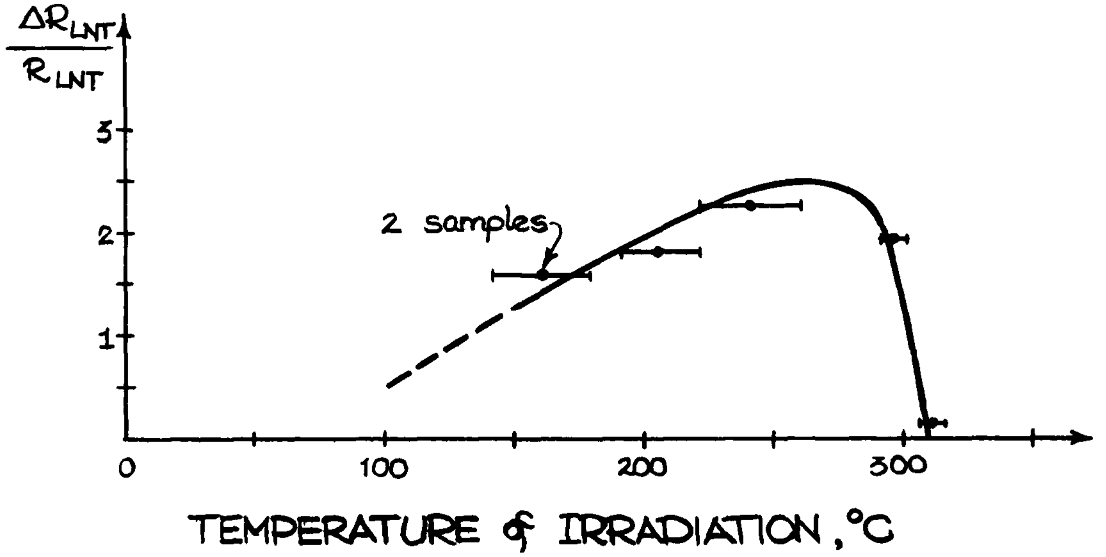
In addition to the microcalorimetry, Ascoli continues to survey other systems using his resistometric technique. He is now scanning Pt doped Au, Au doped Ag and Cu doped Ag. He has ready alloys of Cu doped with Au, Ag and Pt. Finally, he hopes to extend the calorimetry to a study of quenched powders.

Under contract to the National Research Council of Italy Ascoli (and a CNR fellow) has been investigating the reported miscibility gap in the Cu-Ni system. Ascoli had previously studied the rehomogenization kinetics of irradiation-segregated Cu-Ni using resistometric techniques (Physics Letters, 9, No. 4,

p. 305, May 1964) and found that the recovery upon isochronal annealing occured mainly in two stages; one at the temperature ($\sim$350°C) at which vacancy mobility becomes rate controlling and the frozen-in excess vacancies play a role in the redistribution of segregated atoms while they anneal out; the second stage of annealing was found to begin at $\sim$600°C due to the influence of equilibrium vacancies.

However, it was also possible to use the resistometric technique to determine the temperature of the miscibility gap. There are several guesstimates in the literature as to the peak temperature of the gap, the two most reasonable being that of Rapp and Maak (Acta Metallurgica, 10, p. 63, 1962) and that of Meijering (Acta Metallurgica 5, p. 257, 1957) which are 300°C and 180°C, respectively.

To study this reaction, Ascoli, P. Bergamini and B. Bellani have thus far used only one composition, 35.7 a/o Ni. The specimens were irradiated with neutrons (about 4×10^{18} n/cm^2, E $>$ 1 Mev) to introduce defects which would increase the diffusion rate. The γ heating raised the specimen temperature which was then controlled at several preselected temperatures. Following irradiation the resistivity was measured at -196°C. The first data obtained are shown below:



It is difficult to analyze such meager data, but it is, nonetheless, interesting to do so. The data clearly indicate that the clustering accelerated by the excess defects does not occur above 310°C. A peak resistivity effect is observed at about 250°C which then seems to tail off, perhaps suggesting that a lower limit to the gap exists at about 100°C. It is more likely that below 250°C even the many excess vacancies present are not sufficiently mobile to allow the full clustering to obtain. This last idea is in accord with Ascoli's earlier observations. It is also interesting to note that if both this interpretation and that of Rapp and Maak are correct, either a broad flat miscibility gap or one with a peak at about 50 a/o Ni is implied. Ascoli would very much like to continue this work, but he requires outside support before he may do so.

I spoke very briefly to a Dr. Camona, who was to take his professor's examination beginning the next day. He told me about some of their recrystallization studies on pure and thoriated nickel in which they assessed the relations between deformation and recrystallization. They have also recently completed some work on an Al-Mn alloy, again a recrystallization study as a function of cold work for two situations, one where the Mn is in solid solution in the Al and the second where it has been precipitated. This is about to be published in the <u>Materials Science Journal</u>. They find that when the Mn is in solution they obtain the classical structures after various amounts of cold work, eventually getting nicely developed cells with reasonably sharp dislocation walls. However, when the Mn has precipitated, they find that they need more deformation to achieve an equivalent structure, and the cell walls are not as well defined. On the other hand, they found that when the precipitates are present the recrystallization begins in a shorter time for the same amount of cold work, however, secondary recrystallization is retarded.

I talked to Dr. R. Trucco who, working with Dr. E. Cerrai, has developed a laser apparatus which allows them to determine relative grain sizes for annealed materials of different grain sizes and also relative amounts of cold work in materials of the same starting grain size. The technique is to irradiate a given grain (or given area) with a beam of laser light focused through the same optical system used to select the desired area. This vaporizes a small amount of the sample, the vapor is passed through an auxiliary spark immediately above the sample and the light from the sparked vapor goes into a grating spectrograph and is recorded on film. They find that for small grain sizes or large amounts of cold work they get greater line intensity than for large grained or annealed materials. They believe that one has a greater quantity of vapor emitted by a fine grained or cold worked area. This, they think, is because the amount of vapor that comes from a given grain is proportional to the light energy plus the internal energy. This argument may not be tenable; the facts are, however, that such correlations between grain size or cold work and line intensity exist. They have been able to calibrate the intensity of these lines to allow one to measure non-destructively either grain size in various parts of a sample or the amount of residual cold work. They have used this technique with their Al-Mn alloy, zircalloy, 18-8 stainless steel and to study the progress of sintering in copper. The spot size can be made as small as $\sim 10\ \mu m$ with this apparatus. Thus, single grains in most materials can be examined as well as single inclusions. This would seem to be a very interesting development because, except for spot size, the laser microprobe would seem to have none of the disadvantages of the X-ray microprobe. For example, the laser microprobe is cheap, portable, simple and does not require the sample to be placed in an evacuated chamber. Finally, the problems caused by a shallow take-off angle in the X-ray microprobe are also eliminated, although the larger spot size may be a disadvantage. It is clear, though, that this is an excellent poor man's electron probe (X-ray) microanalyzer. Trucco and Cerrai have one published paper which describes this device as it was first developed (la metallurgia italiana, No. 8, p. 615, 1967). They have also prepared a second paper which describes the current modifications and which presents some further results. This latter paper has not yet been released by CISE.

Laboratori di Elettrochimica del Politechnico di Milano

If this paper had been wider, I could have used the full title of this institute, which is: Laboratori di Elettrochimica di Chimica Fisica e di Metallurgia del Politechnico di Milano. This Institute is the private fief of Professor Roberto Piontelli, a well-known and respected electrochemist. I visited his Laboratory not simply for a liaison visit, but because I had heard that he had been completely successful in his attempts to electroplate titanium. He has indeed. The secret is apparently in the electrochemical preparation of the surface.

There are three steps involved. The first step is to remove the natural oxide from the titanium and replace this by another, not fully understood, oxide layer. This is done in a very strong solution of sodium hydroxide (700 grams of sodium hydroxide per liter of water) and requires only 10-20 seconds. The applied voltage must be greater than 30 volts so that some sparking occurs, but not so severe as to cause a crack. This new oxide is black, uniform and apparently gives many TiO_2 lines on X-ray examination -- but it is (apparently) not TiO_2.

The second step is to remove most of the new oxide by treating with alternating current in a hydrochloric acid solution. The voltage is variable and is not too critical. One can use a little lower voltage to slow down the process. This residual film is suitable to prevent reoxidation in handling before plating, but is not sufficient to hinder the perfect adhesion of the electroplate. When the specimen is first immersed in the HCl solution, it bubbles until it (apparently) becomes polarized, although the deplating continues. The trick is to stop deplating before the last of the black oxide is removed (bubbling restarts and TiO_2 forms), while still not leaving very much of it behind. This, they have learned to do through practice, but they did demonstrate how this step may be sensitively controlled with an oscilloscope. This procedure is described completely in Piontelli's first publication on this subject with P. Pedeferri (Electrochimica Metallorum, No. 3, p. 346, 1967). If the natural oxide is allowed to reform, the electroplates will not be perfectly adherent and will flake off during a bending test. If natural oxide does form in the HCl bath one simply goes back to square 1 and begins again.

The third step is to plate on whatever material that you wish. These are all done in standard electroplating baths so there is no difficulty in this step. The Cu, Au and Ag are plated from cyanide baths. The Ni, Co and Rh are plated from sulfamate baths, the Pb from a perchlorate bath, the Pt from a $PtCl_2$ solution and the Cr from the usual solutions, but a drop of fluorides should be added. They have also successfully plated Sn, Cd and Zn and Pb-Cu and Ni-Co alloys. It is quite clear that the materials all plate very well as evidenced by the 180° bending test to which they subjected each one of them to test the true adhesion rather severely. Chromium does not adhere as well as the others, but it can be made considerably better if one flashes the specimen first with Ni or Cu. The usual limit for plating thickness is about $5\mu m$, except for the very soft metals like lead and perhaps gold and silver, which can deform very easily. These can be plated to some greater thickness, but no more than $12\mu m$.

The plates are non-porous and have been subjected to the most severe chemical and electrochemical tests. The mechanical properties have only received a preliminary study, but Piontelli expects to begin a thorough study soon. As examples of the chemical-electrochemical resistance of these electroplates Piontelli has had Pt ($2\text{-}3\mu m$) coated anodes working at 10,000 amp/m^2 in a bath of three-molar HCl saturated with NaCl, at $50^\circ C$: no appreciable evidence of attack after 1000 hours. Tests in NaCl-$NaClO_3$ mixtures at more than 25,000 amps/m^2 at $80^\circ C$ also give fully satisfactory results. The Italian chemical industry is interested, and Piontelli is now working on plating the inside and outside of copper tubes simultaneously with Rh. They are also plating solid specimens which they are going to test in torsion. With a thin layer of electroplate, one very nearly knows the stress in the electroplate, because it is the maximum stress computed at the surface of the torsion specimen. Piontelli has filed for a US patent covering these procedures. Persons interested in testing the quality of simple coupons of titanium variously plated or parties wishing to have specific pieces plated for stateside evaluation should contact the undersigned. It would seem that this simple, cheap technique could greatly increase the usefulness of titanium. For those interested, there are two more extant references: Accademia Nazionale dei Lincei, Serie VIII, Vol. XLII, p. 588, May 1967 and Electrochemical Technology 5, p. 558, Nov-Dec. 1967.

Euratom: Centro Comune di Ricerche, Ispra

This research laboratory, which is now a Euratom community laboratory, was begun as an Italian Government nuclear research laboratory and originally staffed by the CISE nuclear personnel who could now begin to function openly. The first buildings were in Milan, while the site at Ispra was being built. It is notable that the site at Ispra is on the edge of Lake Maggiore in a very beautiful position practically at the base of the pre-Alps. On the weekend one loads skis, kids and car on the ferry boat and in two hours he is in skiing country. The site itself is also very nice, although many of the older buildings are still barracks. The problem with Ispra is that it doesn't really know what its mission is. This will come out in the rest of the discussion. Soon after the laboratory was started, the Italian government began to worry about the expense of maintaining such a large effort, and officially, then, the Government turned the Laboratory over to Euratom without asking the Legislature. There was such a fuss raised about this that they finally did formally go through the Legislature and the transfer to Euratom was approved. In the meantime, (this hung fire for about a year) some of the good people that had come from CISE left because they didn't know where they stood and went to work at the Italian Government nuclear research center at Casaccia, near Rome.

Euratom selected as its mission a reactor development program for an organically cooled reactor. Contrary to the way things are done in the United States, where one designs a reactor to the best of his ability, does some of the research as he does the development, etc., Euratom decided to do all the possible research on the materials and everything else before they began to do the design of the actual reactor. By the time they had reached this happy state of affairs, industry was no longer interested in that type of reactor and so they never did design it. In the meantime, the three European communities; Euratom, the European Economic Community and the European Coal and Iron Community were fused into one, and this fusion is supposed to be finalized in July of this year. At this time, then, the status of Ispra is not known and some of the people there are again beginning to get the jitters. It has been proposed that Ispra should be a general research center rather than a nuclear research center. The six areas that have been suggested for Ispra's specialization are meteorology, environmental effects, automation (computer memories, etc.), materials (high temperature), application of space technology spin-off and new transportation methods. The decision in this regard is to be made by June of this year, and as I said, in the meantime the people sit and wonder.

At Ispra I visited Dr. A. Merlini, who is head of the solid state physics research group, and Dr. Marchetti, who is in charge of the physical chemistry research group, which group is actually doing physical metallurgy research. Merlini's group is mainly concerned with three kinds of research: X-ray scattering effects, point defects and radiation damage in metals and the study of ionic crystals. They have been studying the conductivity of plastically formed alkali halide crystals. It is their contention that the bulk of the defects in these crystals are Frenkel defects. They are studying these crystals by irradiation and determining the number of F centers and other kinds of defects which form. They have found that the formation of F centers is greatly enhanced by the simultaneous application of plastic deformation and X-radiation. One of the fellows that is directly concerned with this work is Ed Saunders, who is spending a year at Ispra on leave from the Oak Ridge National Laboratory. Another piece of research they are doing is to measure the thickness of oxide as the oxides form on uranium monocarbide. This research has apparently just been submitted for publication.

The anomalous transmission of X-rays is a subject that Merlini is interested in himself. The anomalous transmission of X-rays, which was first reported by Walter Bollmann in calcite, has since been found in silicon and germanium, and Merlini's contribution is to prove that this also occurs in zinc. The anomalous transmission occurs in zinc because most of the dislocations lie on the basal plane in this material. Thus, they wouldn't need the dislocation-free nearly perfect crystals required before this phenomenon is observed in other materials. They have used a fairly good zinc crystal oriented so that the basal plane is parallel to the axis which bisects the Bragg angle. The fact that they did get anomalous transmission indicates clearly that the dislocations on the basal plane did not have any effect. However, the magnitude of that transmission was not really quite as much as indicated in the equation that describes this phenomenon. This, they found, is due to the fact that there is both Compton and anelastic scattering in the crystal. Merlini is also doing some work on using the Mössbauer Effect to determine thermal diffuse scattering quantitatively. He has a specimen of Co^{57} decaying to Fe^{57} that is giving low energy γ rays ($E=14.4$ keV, $\lambda = 0.860$Å). If these are diffracted from a crystal (of say silicon), one gets a peak versus the angular orientation of the crystal just as one does for X-rays. If, on the other hand, one interposes an absorber between the crystal and the detector so as to absorb only the Bragg intensity and not the thermal-diffuse-scattering intensity and one moves this absorber back and forth at various velocities, one finds that the intensity transmitted shows a minimum at a certain absorber velocity, which is the resonance velocity. If the absorber is moved at that velocity, it will absorb the Bragg intensity and one obtains a second curve for the same diffraction line, this time with the Bragg intensity removed. One can then make a plot showing the full X-ray peak and the separate magnitudes of the thermal diffuse scattering peak, and the Bragg peak for a given diffraction line. This has worked fairly well for the case of silicon.

I next talked with Dr. C. Bassani. He had spent the year 1961-62 at the Oak Ridge National Laboratory with Wechsler. He has been studying the annealing behavior of solution heat-treated, quenched and irradiated Al-8 ^{a}/o Ag using precision resistivity techniques to follow the changes occurring on annealing. He has found an anomalous resistivity peak at about 105°C which he can in no way understand. This work is continuing, and he will be applying electron microscopy to specimens annealed to selected points in an effort to see how the structure of the irradiated material differs from that of the unirradiated material, which only shows a small peak at 80°C.

I spent the afternoon with Marchetti, who is in charge of the Physical Chemistry Department. This is where the physical metallurgy is all being done. Marchetti gave me a good summary of their recent work and then we went to see the scientists directly involved. SAP (sintered aluminum powder) has been selected as the main structural material for pressure tubes and for canning the elements of the ORGEL reactor (the organic-water reactor that I mentioned before). The mechanical properties of SAP are good to about 150°C, but the creep elongation to rupture is very low. They have now developed a SAP material with about five times the previous elongation at rupture. They did this by first studying the relationship of particle size and oxide platelet size, which showed that finer particles and platelets enhance ductility. Then, they produced this far more ductile SAP material by evaporating a block of aluminum under a cold substrate in the presence of a partial pressure ($\sim 10\,\mu$m) of oxygen. In this way one obtains initial particles of Al of about 100Å diameter and very small oxides. The material is then annealed for a short time and the oxygen diffuses only a short distance. One gets, then, very small oxide particles and Al grains not much bigger than 100Å. This is all the work of Dr. R. Schiller. Schiller has also found that if one anneals a ductile SAP material until the oxide particles grow, the SAP exhibits normal (low) creep ductility again. This occurs regardless of the grain size of the material, the diameter of the specimen or any other variable. Since a specimen of a given grain size will progressively lose creep ductility as the annealing progresses, Schiller feels that the only variable that matters is the oxide particle size.

Marchetti also told me about the preparation of new zirconium alloys to be in contact with the organic coolant selected for the ORGEL reactor. The coolant is therphenyl, which is stable to about 150°C and which shows a lower radiation damage rate than do many other organics. Therphenyl is basically a chain of three benzene rings and the energy of the radiation tends to break the bond between the rings giving fragments of either one or two rings. These, of course, are rather active and they go around and reattach themselves to another fragment and give up the energy, or attach themselves to a molecule that has not been broken up so that some of these combinations can become quite long. Thus, the therphenyl has to be continuously purified to remove the long chain molecules and thus keep the viscosity low. They have found that zircalloy II oxidizes very badly in contact with the irradiated organic. They believe this is due to either the 2 ppm of chlorine or the small amount of water in solution in the therphenyl. They found that a Zr-Fe-Cu ($Cu < 1\%$) alloy is very stable in the presence of this therphenyl. The oxidation of this alloy proceeds very slowly (of the order of a few micrometers per year), the oxide advances inwards as a layer; and they have found that so long as the oxidation continues, the oxide remains impervious to hydrogen which is also produced on the metallic surface during the splitting of water molecules to yield oxygen. But, if the oxidation stops at any time, the oxide becomes permeable to H_2, and the H_2 will diffuse inward and embrittle the Zr alloy. They must also continuously purify the Cl from the therphenyl which they do with Zr getters.

A Dr. Ruedl has been doing electron microscopic studies of these dispersed phase alloys. He has introduced some helium bubbles into SAP in the reactor. Then, if one is very lucky, the contact angle between a bubble and an oxide platelet (against which the bubble finds itself within the bulk of the SAP material) can be measured, and from this measurement one can compute the surface energy. This work is being published in the Journal of Nuclear Materials.

It is known that Mg dissolves very quickly in this organic coolant at about 500°C. Dr. Marchetti's group were attempting to sort out the mechanism of this reaction when they found that 100 ppm of urea in the therphenyl stopped destructive oxidation altogether in two ways. First, it lowered the initial oxidation rate by a factor of about 1000, and second, the oxidation rate becomes parabolic so that the oxidation finally becomes very slow.

Dr. F. Lanza, who has been working on making graphite impermeable, has perfected a technique to pressure inject Mg into the system of interconnective holes in the graphite. They have found that this composite can be used in the range of 300 to 550°C if the impregnation has been successful and fills about 80% of the open pores in the graphite. The Mg-C composite machines very well, is clean, easy to handle and so on. They have developed a technique wherein they take a cylinder of graphite, put a uranium carbide fuel element in it, seal this with graphite plugs and then put the whole thing into a molten Mg bath under pressure. The Mg infiltrates the pores and threaded joints, and when they take it out, it is one solid bar which can be machined to size, etc. Because the Mg permeates through the graphite, there is then a solid Mg metal bond between the uranium carbide and the graphite can. This means that the thermal conductivity between the uranium carbide and the coolant is much better than in the usual case where there is no metallic coupling. This gives about a 200°C lower temperature at the center of the fuel element than with ordinary canning techniques. I believe this technique has several distinct advantages, especially when it is compared to the UK method for making the graphite impermeable (ESN 21-12, 31 December 1967). Lanza has published two papers in this regard: EURATOM Report 2988f, June 1966, and <u>la metallurgia italiana</u> <u>5</u>, p. 364 (1967), and another is in preparation.

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13. ABSTRACT

Research programs in physical metallurgy and solid state physics in three research laboratories in Italy are described. Two not-for-profit laboratories and one university laboratory are included.

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